such, for instance, as magnolia at Gloucester, and great rose-bay at Sebago, — may be stripped of their treasures. These 'late-lingerers' possess great interest, and they should long be carefully guarded. But, so far as our rare plants in general are concerned, we do not yet need any society for their preservation: we do, however, need many local societies for their detection, and for critical study of their habits.

A year ago five commissioners of state water-supply were appointed by the New-Jersey legislature to select the best practicable plans for supplying the cities and towns of the state with pure and wholesome water. A report has recently been presented by them to the governor, on the capabilities of the Passaic-River basin for the collection and storage of water for the several centres of population that must now, or in the near future, depend upon it; and a plan elaborated by Mr. L. B. Ward, hydraulic engineer, is appended for the supply of Jersey City, Newark, and other neighboring municipalities. According to this plan, the waters of the Pequannock, a tributary of the Passaic, can furnish sixty million gallons daily, at an expense of two million dollars. With a further cost of three hundred thousand dollars, the supply can be increased to eighty million gallons, sufficient for all probable requirements for twenty years to come. Farther in the future, the Wanaque and Ramapo watersheds can yield an additional two hundred million gallons daily, so as to serve a population of two million eight hundred thousand souls. The chief danger of pollution in the Pequannock valley is of a modern kind: it comes from leakage of the Oil transit company's pipes that carry petroleum from the oil-wells of Pennsylvania to Jersey City; but this danger can be averted by state enactment. Mr. Ward's report contains a well-prepared contour-line map of the Pequannock basin, with darker and darker tints for every elevation of one hundred feet: this is reproduced from a more extended map, based on 'the valuable contoured maps of the New-Jersey geological survey,' and on special surveys by the commission in the adjacent part of New York. In view of the rapid growth of many of our cities, and of the increasing recognition of the value of a good water-supply, this fore-thoughtful action of the New-Jersey legislature should be imitated in other states.

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Professor Tait on the reality of force.

The arguments by which Professor Tait seeks to disprove the objective reality of force, and to justify his advocacy of the exclusion of the term from scientific writing, occupy two and a half pages at the end of a seventy-four page article on mechanics, in the last edition of the Encyclopaedia Britannica. The vigor and confidence with which they are there stated, notwithstanding the author's treatment of forces as real entities in the body of the article, the character of the publication in which they appear, and the eminence of the Edinburgh professor in mathematics and physics, make them worthy of a careful examination.

In the first place, Professor Tait infers that force can have no such reality as matter has, because it is to be reckoned positively and negatively, — an action being opposed by a reaction, — while matter, or mass, is signless. This suggests two comments: 1°. The author never questions the objective reality of space and time, of which realities it is an essential feature, that, to every direction or interval A–B, an equal direction or interval B–A, of opposite sign, corresponds; 2°. The idea of a negative mass is not a self-contradictory one, and was once widely accepted. The element phlogiston was given up, not because of any absurdity in ascribing levity to material substance, but because a form of matter with positive mass (oxygen), capable of explaining all the phenomena, had been actually separated and identified.

Professor Tait's next criterion of objective reality is quantitative indestructibility, — an attribute shared by time, space, and matter, to which he adds energy. But the evidence of the indestructibility of energy is not of the same nature as that of the indestructibility of matter: for the latter, in all its forms, may be localized, and its density or elasticity measured; while the former, when stored up or 'potential,' cannot be shown to possess a single one of the properties of energy kinetic, or any existence in space, or any objective character whatever. Professor Tait virtually admits this difficulty, and awaits for its solution the discovery of some evidence 'as yet unexplained, or rather unimagined.' All strains and other actions of a clock-weight on its supports are obviously precisely the same — or impalpably somewhat stronger — with the weight wound up an inch as with it wound up a yard; and the existence of a greater 'potential energy' in the latter case is not to be found in the clock, but in the mind, which requires this expression as a form in which to put its conviction that a certain greater amount of work can be obtained. Even though it be admitted that there are no other intelligible terms in which this conviction can be stated, it is clear that the indestructibility of energy is an ideal and subjective truth, and cannot, therefore, be relied on as evidence of a reality distinctively 'objective.'

A third point made by Professor Tait against force is, that its numerical expression is that of two ratios, — the 'space-rate of the transformation of energy,' and the 'time-rate of the generation of momentum.' These results are obtained by simple division, in an equation which expresses the fact that the work done by a body in falling the distance h is just that required to lift it through h against gravity. The fallacy involved in treating the numerical expression for force as force itself has been well exposed by Mr. W. R. Browne, in a criticism of this encyclopaedia article (*Phil. mag.*, November, 1883); and the assumption that ratios are necessarily non-existent is even more fallacious. Were it trustworthy, Professor Tait's equations would lead quite as conclusively to proofs of the non-objectivity of space and time (the former becoming the rate of work-units, the latter of motion-units, per unit of force), and so to a confirmation of the celebrated German view that whatever is universal and necessary in thought belongs to the subject, as to what he deduces from them; or they might even give mass in the form of a ratio, and hence suggest the non-objectivity of matter.

Not the least of the professor's objections against force, it would appear, is that it is 'sense-suggested.' It is a mere truism to say that no other suggestor is possible within the domain of science. It is, perhaps, better worth while to call attention to the indubitable fact that the real ground of the objection against 'action at a distance,' entertained by many physicists, is exactly that such action is not directly suggested by sense-impressions: for this is what they must really mean by calling it 'occult'; actions as our consciousness knows them, and as we can produce them, being generally characterized by proximity undistinguishable from actual contact. Further, if there is any reproach in this epithet, energy is quite as open to it as any function of energy can be: in fact, our senses directly report work in the form of nerve-disturbance, and nothing else. Force is no more truly an inference from nerve-reports testifying of energy exerted, than is matter: in fact, the inference of the independent existence of matter is the less direct and more questionable of the two. The view advocated by Mr. Browne, following Boscovitch, that matter is but 'an assemblage of central forces, which vary with distance, and not with time,' or with direction, is one of great simplicity, as well as suitability to analytic treatment, and one of which no disproof is possible.

It is not too much to claim, therefore, that, in the very difficult task of proving or disproving objective reality, Professor Tait has not here been successful.

HENRY FARQUHAR.

North-eastern and north-western Indian implements.

I do not see that it necessarily follows, because such implements as I have described as 'club-heads' were or are in use among the Ojibwas as 'bone-breakers,' that the Lenni Lenapè used these pebbles for such a purpose, and not in the manner I have suggested. It would naturally be inferred from Miss Babbitt's remarks, that the Dakota puk-gah-mah-gun never varied in its size or shape. If so, then probably no weapons of this pattern have occurred in New Jersey; but this is not true of any form of weapon, agricultural or household implement, ever made by the Indians. They vary indefinitely in size, shape, and degree of finish; and the many forms merge imperceptibly one into the other, as axes into hammers, knives into spears, and these again into arrow-heads. Miss Babbitt herself distinctly states that the two forms of 'club-head' and 'bone-breaker' are essentially the same. If the specimen I figured (fig. 212) in my 'Ancient stone implements of eastern North America' be not a club-head, it does not follow that the more nearly globular fig. 211 was not; and I am glad to be able to state that I have seen just such grooved, globular stones mounted in wooden and hide handles, that were, until very recently, in use by Sioux Indians.

I am very glad that Miss Babbitt has pointed out the use of a large number of these oval, grooved pebbles as 'bone-breakers:' it is a most desirable addition to our knowledge of the archeology of the Atlantic-coast states; and it is now possible to grade and classify this simple pattern of stone implements much more satisfactorily. Of such found in New Jersey, I would say, then, that they are, first, grooved hammers, or mauls; second, club-heads (Dakota, puk-gah-mah-gun); third, 'bone-breakers;' fourth, net-weights.

I suggest this division as based upon the size, the degree of finish, the evidence of use (as in the 'bone-breakers'), and, lastly, the conditions under which many are found. If the flat, discoidal pebbles with side-notches are net-weights, and of this there can scarcely be a doubt, then the smallest of the groove pebbles, which we usually found associated with them, were doubtless put to the same use.

CHARLES C. ABBOTT.

May 18.

Atmospheric waves from Krakatoa.

Mr. H. M. Paul is, doubtless, perfectly correct in insisting (*Science*, iii. 531) that the atmospheric waves following the Krakatoa explosion should not be confounded with the elastic waves producing sounds: in fact, these latter are so brief that it is very questionable whether they would show themselves at all on barometric traces. There would not be time enough for the mercurial barometric column to respond to the momentary compressions and rarefactions: much less would they be indicated by fluctuations extending over thirty minutes or more. The atmospheric waves which encircled the earth, and disturbed the self-registering barometric traces on the 27th of August, 1883, must therefore have been huge aerial gravity-waves, due to the enormous displacement of air produced by the ejection of vast volumes of gaseous products into the atmosphere at the time of this volcanic explosion: they were analogous to the great earthquake water-waves that are sometimes transmitted thousands of miles across oceans.

The point in this connection which needs elucidation is the fact, established by the observations of Gen. Strachey, Professor Förster, and others, that the velocity of these waves was approximately the same as that of an elastic sound-wave in air. It is the near coincidence of these velocities which has led to the confounding of these gravity-waves with elastic sound-waves. The approximate identity of the velocities in these two cases may be traced to the relation existing between the elasticity or resilience of the air, on which the velocity of sound depends, and the height of a homogeneous atmosphere, on which the velocity of long aerial gravity-waves depends.

It is well known that the mathematical investigations of Sir G. B. Airy and others, confirmed by the experimental results of Scott Russell, show, that, in the class of water-waves in which the wave-length bears a large ratio to the mean depth of the water, the velocity of propagation of the wave is sensibly